eighth grain of morphia; wound discharging laudable pus; temperature, 101½; pulse, 110; respiration, 24.

July 10.—Restless night, no morphia being given. Wound still discharging healthy pus. Temperature, 101½; pulse, 110; respiration, 26.

July 11.—Temperature, 101; pulse, 110; respiration, 24.

July 12.—Temperature, 102; pulse, 110; respiration, 22. Ordered digitalis.

July 13.—Temperature, 100½; pulse, 100; respiration, 22. Urine containing traces of albumen. Solid food taken and retained.

July 14.—Temperature, 101; pulse, 108; respiration, 20.

July 15.—Temperature, 100½; pulse, 95; respiration, 22.

July 16.—Temperature, 100½; pulse, 100; respiration, 22.

July 17.—Temperature, 101; pulse, 100; respiration, 23.

July 18.—Temperature, 101; pulse, 100; respiration, 22.

July 19.—Very restless night. Temperature, 101½; pulse, 130; respiration, 34. Complains of pain in the region of the heart.

July 20.— Temperature 101½; pulse, 120; respiration, 34,

July 21.—Temperature, 101; pulse, 112; respiration, 32.

July 23.—Restless night, troubled much by a short, hacking cough; wound entirely healed. Temperature, 100¾; pulse, 106; respiration, 32. Vomited his breakfast.

July 24.—Passed a restless night notwithstanding the free use of bromide· Temperature, 103; pulse, 130; respiration, 38. Still troubled with cough, which distresses him greatly; cannot retain solid food. Stimulants freely given.

July 25.—Slept better, but cough still troubles him; breathing labored. Temperature, 100¾; pulse, 65; respiration, 39; muscular twitching of hands and feet.

July 26.—Much more comfortable this morning. Temperature, 100½; pulse, 92; respiration, 40; digitalis discontinued.

July 27.—Temperature, 99; pulse, 58; respiration. 36.

July 28.—Temperature, 98½; pulse, 56; respiration, 30.

July 29.—Temperature, 99; pulse. 60; respiration. 32.

July 30.—Delirious during the night, attempted to get out of bed. Temperature, 99½; pulse, 52; strong and full; respiration, 28.

July 31.—Temperature 99½; pulse, 68; respiration, 32. Delirious during night. Bromides given freely.

August 1.—Temperature, 100; pulse, 52; strong and full; respiration, 34.

August 2.—Temperature, 98½; pulse, 51; respiration. 30. Delirious during night.

August 3.—Temperature, 98½; pulse, 108; respiration, 22. Troubled very much with attacks of coughing.

August 4.—Temperature, 98½; pulse, 100; respiration, 24.

August 5.—Temperature, 98½; pulse, 96; respiration, 24.

August 6.—Temperature, 100; pulse, 96; respiration, 20.

August 7.—Temperature, 99; pulse 94; respiration 19.

August 8—Temperature, 98¾; pulse, 88; respiration, 22; sleeps well; appetite, good.

August 9.—Temperature, 98½; pulse, 90; respiration, 20.

August 13.—Temperature, pulse and respiration have remained the same as on August 9. The patient for the first time to-day since his injury has been allowed to get up and dress.

August 18.—Doing well since last report. Walks around the wards; eats and sleeps well, the bullet remaining in his body.

ON THE GERM THEORY.*

By Prof. Pasteur.

"The subject of my communication is vaccination in relation to chicken cholera and splenic fever, and a statement of the method by which we have arrived at these results—a method the fruitfulness of which inspires me with boundless anticipations. Before discussing the question of splenic fever vaccine, which is the most important, permit me to recall the results of my investigations of chicken cholera. It is through this inquiry that new and highly important principles have been introduced into science concerning the virus or contagious quality of transmissible diseases. More than once in what I am about to say I shall employ the expression virus-culture, as formerly, in my investigations on fermentation, I used the expressions, the culture of milk ferment, the culture of the butyric vibrion, etc. Let us take, then, a fowl which is about to die of chicken cholera, and let us dip the end of a delicate glass rod in the blood of the fowl with the usual precautions, upon which I need not here dwell. Let us then touch with this charged point some *bouillon de poule*, very clear, but first of all rendered sterile under a temperature of about 115° centigrade, and under conditions in which neither the outer air nor the vases employed can introduce exterior germs —those germs which are in the air, or on the surface of all objects. In a short time, if the little culture vase is placed in a temperature of 25° to 35°, you will see the liquid become turbid and full of tiny microbes, shaped like the figure 8, but often so small that under a high magnifying power they appear like points. Take from this vase a drop as small as you please, no more than can be carried on the point of a glass rod as sharp as a needle, and touch with this point a fresh quantity of sterilized *bouillon de poule* placed in a second vase, and the same phenomenon is produced. You deal in the same way with a third culture vase, with a fourth, and so on to a hundred, or even a thousand, and invariably within a few hours the culture liquid becomes turbid and filled with the same minute organisms.

"At the end of two or three days' exposure to a temperature of about 30° C. the thickness of the liquid disappears, and a sediment is formed at the bottom of the vase. This signifies that the development of the minute organism has ceased—in other words, all the little points which caused the turbid appearance of the liquid have fallen to the bottom of the vase, and things will remain in this condition for a longer or shorter time, for months even, without even the liquid or the deposit undergoing any visible modification, inasmuch as we have taken care to exclude the germs of the atmosphere. A little stopper of cotton sifts the air which enters or issues from the vase through changes of temperature. Let us take one of our series of culture preparations—the hundredth or the thousandth, for instance—and compare it in respect to its virulence with the blood of a fowl which has died of cholera; in other words, let us inoculate under the skin ten fowls, for instance, each separately with a tiny drop of infectious blood, and ten others with a similar quantity of the liquid in which the deposit has first been shaken up. Strange to say, the latter ten fowls will die as quickly and with the same symptoms as the former ten; the blood of all will be found to contain after death the same minute infectious organisms. This equality, so to speak, in the virulence both of the culture preparation and of the blood is due to an apparently futile circumstance. I have made a hundred culture preparations—at least, I have understood that this was done—without leaving any considerable interval between

* "International Medical Congress." London, 1881.

the impregnations. Well, here we have the cause of the equality in the virulence.

"Let us now repeat exactly our successive cultures with this single difference, that we pass from one culture to that which follows it—from the hundredth to, say, the hundred and first, at intervals, of a fortnight, a month, two months, three months or ten months. If, now, we compare the virulence of the successive cultures, a great change will be observed. It will be readily seen from an inoculation of a series of ten fowls that the virulence of one culture differs from that of the blood and from that of a preceding culture when a sufficiently long interval elapses between the impregnation of one culture with the microbe of the preceding. More than that, we may recognize by this mode of observation that it is possible to prepare cultures of varying degrees of virulence. One preparation will kill eight fowls out of ten, another five out of ten, another one out of ten, and another none at all, although the microbe may still be cultivated. In fact, what is no less strange, if you take each of these cultures of attenuated virulence as a point of departure in the preparation of successive cultures and without appreciable interval in the impregnation, the whole series of these culures will reproduce the attenuated virulence of that which has served as the starting point. Similarly, where the virulence is null it produces no effect. How, then, it may be asked, are the effects of these attenuating virulences revealed in the fowls? They are revealed by a local disorder, by a morbid modification more or less profound in a muscle, if it is a muscle which has been inoculated with the virus. The muscle is filled with microbes which are easily recognized, because the attenuated microbes have almost the bulk, the form, and the appearance of the most virulent microbes.

"But why is not the local disorder followed by death? For the moment let us answer by a statement of facts. They are these: the local disorder ceases of itself more or less speedily, the microbe is absorbed and digested, if one may say so, and little by little the muscle regains its normal condition. Then the disease has disappeared. When we inoculate with the microbe, the virulence of which is null, there is not even local disorder, the *naturæ medicatrix* carries it off at once, and here, indeed, we see the influence of the resistance of life, since this microbe, the virulence of which is null, multiplies itself. A little farther, and we touch the principle of vaccination. When the fowls have been rendered sufficiently ill by the attenuated virus which the vital resistance has arrested in its development, they will, when inoculated with virulent virus, suffer no evil effects, or only effects of a passing character. In fact, they no longer die from the mortal virus, and for a time sufficiently long, which in some cases may exceed a year, chicken cholera cannot touch them, especially under the ordinary conditions of contagion which exist in fowl-houses. At this critical point of our manipulation—that is to say, in this interval of time which we have placed between two cultures, and which causes the attenuation—what occurs? I shall show you that in this interval the agent which intervenes is the oxygen of the air. Nothing more easily admits of proof. Let us produce a culture in a tube containing very little air, and close this tube with an enameller's lamp. The microbe in developing itself, will speedily take all the oxygen of the tube and of the liquid, after which it will be quite free from contact with oxygen. In this case it does not appear that the microbe becomes appreciably attenuated, even after a great lapse of time. The oxygen of the air, then, would seem to be a possible modifying agent of the virulence of the microbe of chicken cholera—that is to say, it may modify more or less facility of its development in the body of animals. May we not be here in presence of a general law applicable to all kinds of virus? What benefits may not be the result? We may hope to discover in this way the vaccine of all virulent diseases; and what is more natural than to begin our investigation of the vaccine of what we in French call charbon, what you in England call splenic fever, and what in Russia is known as the Siberian pest, and in Germany as the Milzbrand.

"In this new investigation I have had the assistance of two devoted young *savants*—MM. Chamberland and Roux. At the outset we were met by a difficulty. Among the inferior organisms, all do not resolve themselves into those corpuscle germs which I was the first to point out as one of the forms of their possible development. Many infectious microbes do not resolve themselves in their cultures into corpuscle germs. Such is equally the case with beer yeast which we do not see develop itself usually in breweries, for instance, except by a sort of scissiparity. One cell makes two or more, which form themselves in wreaths; the cells become detached, and the process recommences. In these cells real germs are not usually seen. The microbe of chicken-cholera and many others behave in this way, so much so that the cultures of this microbe, although they may last for months without losing their power of fresh cultivation, perish finally like beer yeast which has exhausted all its aliments. The anthracoid microbe in artificial cultures behaves very differently. In the blood of animals, as in cultures, it is found in translucid filaments more or less segmented. This blood or these cultures freely exposed to air, instead of continuing according to the first mode of generation, show at the end of forty-eight hours corpuscle germs distributed in series more or less regular along the filaments. All around these corpuscles matter is absorbed. as I have represented it formerly in one of the plates of my work on the diseases of silkworms. Little by little all connection between them disappears, and presently they are reduced to nothing more than germ dust.

"If you make these corpuscles germinate, the new culture reproduces the virulence peculiar to the thready form which has produced these corpuscles, and this result is seen even after a long exposure of these germs to contact with air. Recently we discovered them in pits in which animals dead of splenic fever had been buried for twelve years, and their culture was as virulent as that from the blood of an animal recently dead. Here I regret extremely to be obliged to shorten my remarks. I should have had much pleasure in demonstrating that the anthracoid germs in the earth of pits in which animals have been buried are brought to the surface by earthworms, and that in this fact we may find the whole etiology of disease, inasmuch as the animals swallow these germs with their food. A great difficulty presents itself when we attempt to apply our method of attenuation by the oxygen of the air to the anthracoid microbes. The virulence establishing itself very quickly, often after twenty-four hours in an anthracoid germ which escapes the action of the air, it was impossible to think of discovering the vaccine of splenic fever in the conditions which had yielded that of chicken-cholera. But was there, after all, reason to be discouraged? Certainly not; in fact, if you observe closely, you will find that there is no real difference between the mode of the generation of the anthracoid germ by scission and that of chicken-cholera. We had therefore reason to hope that we might overcome the difficulty which stopped us by endeavoring to prevent the anthracoid microbe from producing corpuscle germs, and to keep it in this condition in contact with oxygen for days, and weeks, and months. The experiment fortunately succeeded.

"In the ineffective (*neutre*) *bouillon de poule* the anthracoid microbe is no longer cultivable at 45° C. Its culture, however, is easy at 42° or 43°, but in these conditions the microbe yields no spores. Consequently it is possible to maintain in contact with the pure air at 42° or 43° a *mycélienne* culture of bacteria entirely free of germs. Then appear the very remarkable results which follow. In a month or six weeks the culture dies—that

is to say, if one impregnates with it fresh *bouillon*, the latter is completely sterile. Up to that time life exists in the vase exposed to air and heat. If we examine the virulence of the culture at the end of two days, four days, six days, eight days, etc., it will be found that long before the death of the culture the microbe has lost all virulence, although still cultivable. Before this period it is found that the culture presents a series of attenuated virulences. Everything is similar to what happens in respect to the microbe in chicken cholera. Besides, each of these conditions of attenuated virulence may be reproduced by culture; in fact, since the charbon does not operate a second time (*ne récidive pas*), each of our attenuated anthracoid microbes constitutes for the superior microbe a vaccine—that is to say, a virus capable of producing a milder disease. Here, then, we have a method of preparing the vaccine of splenic fever. You will see presently the practical importance of this result, but what interests us more particularly is to observe that we have here a proof that we are in possession of a general method of preparing virus vaccine based upon the action of the oxygen and the air—that is to say, of a cosmic force existing everywhere on the surface of the globe.

"I regret to be unable, from want of time, to show you that all these attenuated forms of virus may very easily, by a physiological artifice, be made to recover their original maximum virulence. The method I have just explained of obtaining the vaccine of splenic fever was no sooner made known than it was very extensively employed to prevent the splenic affection. In France we lose every year, by splenic fever, animals of the value of twenty million francs. I was asked to give a public demonstration of the results already mentioned. This experiment I may relate in a few words. Fifty sheep were placed at my disposition, of which twenty-five were vaccinated. A fortnight afterward the fifty sheep were inoculated with the most virulent anthracoid microbe. The twenty-five vaccinated sheep resisted the infection; the twenty-five unvaccinated died of splenic fever within fifty hours. Since that time my energies have been taxed to meet the demands of farmers for supplies of this vaccine. In the space of fifteen days we have vaccinated in the departments surrounding Paris more than twenty thousand sheep, and a large number of cattle and horses. If I were not pressed for time I would bring to your notice two other kinds of virus attenuated by similar means. These experiments will be communicated by-and-by to the public. I cannot conclude, gentlemen, without expressing the great pleasure I feel at the thought that it is as a member of an international medical congress assembled in England that I make known the most recent results of vaccination upon a disease more terrible, perhaps, for domestic animals than small-pox is for man. I have given to vaccination an extension which science, I hope, will accept as a homage paid to the merit and to the immense services rendered by one of the greatest men of England, Jenner. What a pleasure for me to do honor to this immortal name in this noble and hospitable city of London!"

FROM a privately issued report on silk cultivation in the Chinese province of Kwangtung, we learn that in the Pakhoi district, on the southern seaboard, wild silkworms are found which feed on the camphor tree, and their silk is utilized in a singular manner. When the caterpillar has attained its full size, and is about to enter the *pupa* state, it is cut open and the silk extracted in a form much resembling catgut. This substance, having undergone a process of hardening, makes excellent fish line, and is generally used for that purpose in the Pakhoi district.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE."

Mr. Samuel J. Wallace, commenting on my paper on "The Use of Water as a Fuel" ("SCIENCE," Vol. II., p. 321), in an interesting communication to you ("SCIENCE," Vol. II., p. 373), suggests an inadvertancy on my part in "not more clearly distinguishing between the degrees of temperature at which the transfer of oxygen takes place from the hydrogen of the water to the carbon set free by the dissociation of the naphtha and the number of heat units set free or absorbed by such transfer, which is a very different thing."

To this I would state in reply that I have purposely refrained from an elaborate calculation of the thermal effects in heat units for several reasons. Of these I shall detail but a few of the more important at present.

In the first place, my intention was to give the scientific rationale of the chemical processes involved in the generation of the tremendous heat produced by the Holland retort with so insignificant an amount of naphtha; and, furthermore, I wanted to show that the application of the principle of the correlation of forces and conservation of energy to this new and original process of combustion has been undertaken heretofore on an erroneous assumption; lastly, I intended to prove, in the shortest and clearest possible manner, what a proportion of heat was gained, and in what manner—viz., by the dissociation of steam in the presence and by the agency of the carbon contained in the naphtha.

For these and other reasons, I avoided long explanations and calculations of other points, such as, for instance, the "dissociation of the naphtha," as Mr. Wallace puts it, and the figuring up of the heat units generated by the several elements on combustion. In order to re-affirm my position, which is, on most points, not that assumed by Mr. Wallace, I may be allowed to offer the following remarks:

It is self-evident that the carbon of the naphtha, in order to act independently, must first be set free; this is accomplished by the heating of the naphtha, in its chamber of the retort, up to the point of gasification. On meeting the steam in the manifold, the carbon of the naphtha leaves its hydrogen and forthwith unites with the oxygen of the watery vapor, forming *either* carbonic oxide *or* carbonic acid, according to the amount of steam introduced.

Thus there is certainly a decomposition of the naphtha into its elements, as Mr. Wallace intimates; but by far the most important process is the *dissociation of the watery vapor* which Mr. Wallace refuses to recognize, insisting, as he does, that there is only a *transfer* of the oxygen from the hydrogen of the steam to the carbon of the naphtha. How this is possible, without the previous dissociation of the steam, I am unable to understand. Mr. Wallace furnishes, indeed, the best argument against his own statement, by mentioning the well-known fact that the carbon in the naphtha is very loosely held by its hydrogen. But it is also a well-known fact that the oxygen of the steam is very tenaciously held by its hydrogen, so much so that it was considered impossible to separate, *to dissociate*, them by heat for a long time. Not until the late Henri St. Clair Deville * devised an appa-

* It is with profound grief that the announcement of the great chemist's death has been received everywhere. At his funeral (July 5th) M. Pasteur made an eloquent speech. The London *Chemical News* has an obituary in which occurs the following passage: "His highest achievement, from a strictly scientific point of view, was the establishment of the laws of dissociation. Previously, decomposition was regarded as a simple phenomenon, effected and completed, in the case of every substance, at a fixed temperature. Deville showed that in some cases it is effected within certain limits of temperature, being arrested at a given heat by the equilibrium established between the decomposing body and the product of decomposition."